LETTERS TO THE EDITOR.

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Further Discovery of Dodos' Bones.

SINCE the astonishing discovery, in 1865, of innumerable bones of the dodo in the peat of the Mare aux Songes by Mr. George Clark, of Mahébourg, in Mauritius (*Ibis*, 1866, pp. 141–146), whereby Prof. Owen was enabled to describe the greater part of the skeleton of that remarkable bird (*Trans.* Zool. Soc., vi. pp. 49–80), and the subsequent researches at the same place of Mr. Sauzier in 1889, the results of which, when worked out by Sir Edward Newton and Dr. Gadow (*Trans.* Zool. Soc., xiii. pp. 281–302), almost wholly completed our knowledge of its osteology—besides affording evidence of the former existence of other contemporary species now extinct—nothing more has been recorded on the subject.[1] It was therefore with great interest that, just five years ago, October, 1899, I received a letter from M. E. Thirioux informing me of his having found, in the preceding month of August, some remains of at least two dodos in a small, partly collapsed cave, about 800 feet above the sea, and about two miles and a half from Port Louis. Encouraged by this success M. Thirioux continued his operations, a matter of some difficulty, not to say danger, from time to time, and was good enough to keep me acquainted with many of the results, sending me photographs of the bones which he was fortunate in disinterring from the soil. They were not all dodos' bones, but some belonged to other extirpated forms of birds—as the brevipennate parrot (Lophopsittacus), the "Poule Rouge" (Aphanapteryx), and the coot—and reptiles—as Didosaurus and one or more of the land tortoises—all of which are very imperfectly known, while some of the small dodo bones are of great rarity, and at least one of them (the pygostyle) had not been seen before. From that time until very recently M. Thirioux has been continuing his researches, and has consequently formed a very considerable collection, which he now writes to me he has disposed of to the Museum of Mauritius, and I can but express the fervent hope that some competent person may be found to work it out and publish a memoir on it which will be a worthy successor to those that I have already mentioned. ALFRED NEWTON.

Cambridge, October 20.

The Forest-pig of Central Africa.

THERE are two good mounted specimens of the forest-pig in the Museum of the Congo Free State at Tervueren, near Brussels, where I had the pleasure of examining them in July last. M. A. Dubois, conservator of the Royal Museum of Natural History at Brussels, told me that he intended to describe the animal in conjunction with Dr. Matschie, of Berlin, but I am not aware that their description has yet been published, so that I hope the forest-pig may remain known by the excellent name Hylochœrus, proposed for it by Mr. Thomas.

As regards the "third mysterious animal" of the Congo Forest alluded to by Sir Harry Johnston in his letter on this subject (NATURE, p. 601), I have little doubt that it was the fine antelope of the genus Tragelaphus, lately described by Mr. Thomas as *Baeocephalus euryceros isaacsoni* (*Ann. Nat. Hist.* (7), v. p. 310, and *Proc.* Zool. Soc., 1902, ii. p. 319). The first pair of horns of this species was obtained by Mr. F. J. Jackson in 1897 (see *Proc.* Zool. Soc., 1897, p. 455), but it is only recently that the perfect specimen which now adorns the mammal gallery of the British Museum was procured.

The "abnormally developed horns of the cow eland" referred to by Sir Harry Johnston have nothing to do with this antelope. They will be found fully described and figured in the "Book of Antelopes" (vol. iv. p. 209).

P. L. SCLATER.

[1] Some reputed dodos' bones, said to have been found in a cavern (*Proc.* Zool. Soc., 1885, p. 719), turned out to be turkeys' (*op. cit.*, 1890, p. 402).

Average Number of Kinsfolk in each Degree.

THE letter you forward to me from Prof. G. H. Bryan gives an opportunity of discussing the question somewhat more thoroughly than space allowed in my brief memoir of September 29.

The writer says:—"Is Dr. Galton's deduction of $d-\frac{1}{2}$ correct? I should have thought that if a parent had d male and d female children, each female child would have $d-1$ sisters and d brothers."

The objection holds good only on the erroneous supposition that each and every family of $2d$ children consists of d boys and d girls; it does not hold good on my supposition that each such family contains *on the average* d boys and d girls. The inclusion of the omitted word introduces a new set of considerations. They depend on the variety of the possible forms of combination of boys and girls in $2d$ children, which are $2d+1$ in number, and on the frequency of each of these forms, which is given by the $d+1$ terms of the binomial expansion of $(1+1)^{2d}$. The exact character of the process concerned is clearly appreciated by thoroughly working out some particular case, say that of $d=2\frac{1}{2}$, where the number of children, $2d$, in each family will be 5. There are then 6 possible combinations of boys and girls, forming 6 different classes; shown in the first three lines of the table.

(1) Classes	I.	II.	III.	IV.	V.	VI.	
(2) Boys in each family	5	4	3	2	1	0	
(3) Girls in each family	0	1	2	3	4	5	
(4) Sisters in each family ...	—	—	2	6	12	20	Totals
(5) No. of families in each class	1	5	10	10	5	1	32
(6) Girls in all the families ...	—	5	20	30	20	5	80
(7) Sisters in all the families...	—	—	20	60	60	20	160

In line (4) is shown the number of sisters in any *one* family of each of these classes ($n.(n-1)$ sisters to n girls). Thus in each family in class vi. there are 5 girls, consequently $5\times4=20$ sisters, in class v. there are 4 girls, and $4\times3=12$ sisters, and so on. The total number of combinations of boys and girls in a family of 5 children $=2^5=32$, which are distributed into six classes according to the familiar binomial fashion as above; these are shown in line (5). Multiplying each entry in (5) with that in the same column in (3) we obtain line (6), which shows that the total number of girls in the 32 families is 80 ($=2\frac{1}{2}\times16$, as it should be). Multiplying similarly the entries in (5) by those in (4) we obtain line (7), which shows that the 80 girls have between them 160 sisters; consequently each girl has on the average 2 sisters. This is identical with my $d-\frac{1}{2}$.

I have made similar calculations for values of $d=1$, $1\frac{1}{2}$, 2, $2\frac{1}{2}$ (above), and 3. In each case the result is that a girl has on the average $d-\frac{1}{2}$ sisters. It may therefore be assumed that the reasoning by which I originally arrived at that deduction is correct.

Before concluding, I should like to direct attention to a slip of the pen in the last line but one of my memoir, which somehow escaped correction; the term $d=5$ should have been $2d=5$. The context corrects the mistake, which may nevertheless puzzle the reader for a while.

FRANCIS GALTON.

Mendel's Law.

IN his letter of last week detailing his most interesting experiments on cross-bred maize, Mr. R. H. Lock makes the following statement:—"I see from the published account of a recent discussion at the Cambridge meeting of the British Association that the facts of Mendelian segregation are still disputed by the biometric school of evolutionists." Now it is easy to make a general statement about some vaguely defined group of men, and I have no right to speak for biometricians as a body. But as inventor of the term *biometry*, I may perhaps be allowed to say what I understand by it as a science, and to restate what I said with some emphasis at the Cambridge meeting. Biometry is only the application of exact statistical methods to the problems of biology. It is no more pledged to one hypo-

thesis of heredity than to another, but it must be hostile to all treatment which uses statistics without observing the laws of statistical science. The criticism which has been published in *Biometrika* upon Mendelian work has attacked its too frequent want of method and of logic, and I think no one can have read recent literature without seeing that the criticism has been effective in its aim. Even Prof. Tschermak now allows a large influence to ancestry, although he asserts that the offspring are not distributed "in the proportions of Galton and Pearson." As I have never distributed the offspring in *fixed* proportions, I may perhaps be content with the admission.

I have headed my letter "Mendel's Law," but the difficulty is to know what is understood by this term. Mr. Lock reproves me in his "Studies in Plant Breeding in the Tropics" because I distinguished a theory of the pure gamete from pure Mendelianism, for I thought, and still think, Mendel himself considered "dominance" an essential part of his system. Another Mendelian protagonist, Prof. Castle, in his last paper writes:—"The basic principles of Mendel's law are two, the principle of dominance and the principle of segregation." Which view is the correct one? If Mendel's law be limited to its earliest form, then it may cover Mendel's own observations and Mr. Lock's maize, but there are many other cases of segregation which it does not cover. So far as I am aware, the only attempt to carry out any form of Mendelianism to its logical conclusion was produced by one biometrician at the suggestion of a second. I refer to my memoir in the *Phil. Trans.*—"A Generalised Theory of Alternative Inheritance with Special References to Mendel's Laws." Even then we did not succeed in making the fundamental hypotheses wide enough to cover the case of man, but we did show—what must be obvious on consideration—that a *description* by modern statistical methods of actual observations need not, as such, be itself opposed to any physiological hypothesis. Out of Mendelianism came on analysis the condemned "law of regression" and the diminishing correlations of the "ancestral law" whenever a population springing from hybrids mated at random.

One might at least have hoped that this result would have demonstrated how idle it is to contrast a school of "Mendelians" with one of "Ancestrians." It is, I fear, however, vain for the biometrician to try and right himself with the non-mathematically trained biologist. Notwithstanding that in every generation dealt with in my memoir the fundamental idea of Mendel is accepted and the re-crossing of the parental forms with each member of the generation occurs and is treated as giving its Mendelian result, Mr. Lock in his "Studies in Plant Breeding" states that I entirely ignore Mendel's demonstration of the truth of his hypothesis by the process of recrossing with the parental form. The only ignoration seems to be one on Mr. Lock's part of what lies behind the mathematical symbols. What, then, is the Mendel's law for which Mr. Lock provides a "crucial experiment"? The mere fact of segregation? Two grey-eyed human parents will produce blue- and brown-eyed children; this has been long known, and is equally crucial. The segregation of *recessives* in certain cases in the proportion of a quarter? This is a fact, but, accepting the fact, is it needful to accept Mendel's theory to describe it? For Mr. Lock's maize, as for mice, we may fairly ask where the other homozygote is before we accept the experiments even as complete cases of the old simple Mendelianism. But Mr. Lock tells us that not even in 1900 did Mendelians suppose Mendel's law to hold good for all characters in all species. The experiment is therefore clearly not "crucial" for heredity at large. It is of interest, great interest, as adding to the number of things in which a Mendelian proportion of 1 in 4 holds for recessives. Will anyone explain why the absence of colour bulks at present so largely in the characters for which this proportion holds? There must be some physiological ground for it.

KARL PEARSON.

The Formation of Polonium from Radium.

THE idea has for some time been afloat that the polonium found in radio-active minerals is a product of the radium that they contain. I have recently made an experiment which seems to afford considerable evidence that this is the case. Some radium salt of quite low activity (barely sufficient to produce fluorescence), which has been in my possession four years or more, was dissolved in water, and some cupric chloride added. The solution was precipitated with sulphuretted hydrogen (the copper served to give a manageable quantity of precipitate).

The sulphide was very active. It was dissolved in nitric acid, and a plate of bismuth immersed in the solution, in order to collect polonium, after Marckwald. This plate became intensely active, giving α rays only. The activity was sufficient to light up a blende screen. The rays showed diminished penetrating power the further they had penetrated; their initial penetrating power was exactly the same as that of the rays of the polonium from pitchblende.

I think it will be agreed that the activity of this bismuth plate may be regarded as due to polonium. Its activity has not yet diminished. The question remains, was this polonium part of the original mineral, or has it been generated since? It is difficult to believe that the radiferous barium could have been freed from copper, bismuth, and the other metals in pitchblende, without being freed from polonium too.

I am making fresh experiments to see whether the formation of polonium can be traced in a radium solution initially quite free from it.

R. J. STRUTT.

Terling Place, Witham, Essex.

Misuse of Words and Phrases.

UNFORTUNATELY a good style of writing English is not a strong point among men of science, especially mathematicians. The chief defects may, I think, be classed under three heads. First, grammatical errors, such as *Bessel functions, the Faraday effect, an uniform density* instead of *Bessel's functions, Faraday's effect* and *a uniform density*. Secondly, the use of uncouth, inelegant, and inaccurate phrases, such as *coal-stuff-gas, stretch-squeeze ratio, non-singular cubic or quartic curve*. Thirdly, a vague, obscure and slovenly mode of constructing sentences, whereby the author envelops his meaning in a cloud of mystery instead of enlightening the understanding of his readers. In fact, the sentences of some authors are so inartistically worded as to produce the impression that they labour under the delusion that a vague and obscure style of writing is evidence of profundity, whilst a clear and lucid one betokens shallowness.

The English language is by no means an easy one to write clearly and concisely, which is due to various causes, amongst which may be mentioned the absence of declensions. In Latin the nouns to which two pronouns respectively refer are always known if (as frequently happens) their genders are different; but in English considerable care is often required in the arrangement of a sentence so as to avoid ambiguity.

As regards the choice of language, there are two cardinal rules to be observed. In the first place, words are to be construed according to their natural and literal meaning unless there is something in the context to show that they are used in an artificial or secondary sense; secondly, lucidity and brevity ought always to be aimed at, and circumlocution and verbosity avoided.

October 22. A. B. BASSET.

The British Association and Referees.

THE correspondence in NATURE some time ago respecting referees induces me to send you the following singular example of their unbusiness-like ways in the hope that greater care may be exercised in the future.

I submitted a radium paper to Section B for the Southport meeting. It was accepted; the usual proof was printed, revised by me and returned. *At* Southport it was decided by a joint committee of Sections A and B that the radium papers held by the latter should be handed over to Section A. This was done. It appears that my paper, now in new hands, was submitted to a referee and condemned. At the close of the meeting I was informed of the fact by the assistant general secretary. In the meantime, however, in reply to my personal inquiries, I had become acquainted with the state of things, and ventured partly to express my views on radio-activity at the discussion in Section A. In the sub-